whose heads were not ready. I prefer the former hypothesis, for it is not, in this case, the inheritance of characters casually acquired by a few chance individuals, but of characters which have been regularly acquired by the race for several generations as an active organic adaptation to the environment.

It is needless here to multiply illustrations, but this and kindred cases have led me to ask whether too much attention has not been paid to single organisms and the modifications of chance individuals, whereas if a whole colony of similar organisms invade a new region, all have to adopt new habits of life; the young members of the colony and the new young continually born, being plastic, will, before they are adults, show marked structural modifications in adaptation to these new habits. They are actively modified by these functional adaptations to their environment. Natural Selection will winnow out those who do not keep in training, and at the same time infallibly compel the successive generations to perfect themselves earlier and earlier. We should thus have stages (but without appreciable breaks in the sequence) in this evolutionary process. (1) The young are born and continue for some time like the ancestors of the group, while the adults show considerable structural modification; (2) the new-born young resemble the ancestors of the group, but commence early to show the functional adaptations of the adults; (3) the new-born differ slightly from the ancestors of the group, inheriting (or possessing "accidentally") slight modifications in the right direction which enable them still earlier to perfect the necessary adaptation; (4) the young are born with the structures necessary to the immediate adoption of the habits required by the environment.

It seems to me that, in this way, we actually have the transmission of acquired characters by inheritance, this inheritance coming in as a natural term at the end of a long series of individual acquirements. The prime factor in the evolution of new forms is, therefore, the vital response to the environment of living colonies of exquisitely sensitive organisms; Natural Selection not only perfects, but drives the resulting modifications back earlier and earlier in the life-history of each individual of the colony *until they are inherited.*

This principle, it seems to me, explains the degeneration of structures which are "only passively functional," such as a hard shell. If the environment no longer requires a shell, it will not be maintained. It is inherited, say, from ancestors, but each fresh generation, or in the case of the Crustacea, each moult, gives a new start. If the shell owed its origin to the inorganic environment, it would cease to be developed when the special stimulus to its production were withdrawn, for the skin which secretes the shell is passively functional only so far as the organism is concerned; as a living tissue it is actively responsive to its environment. Or again, the shell owed its origin to the protection it afforded from enemies, and thus partly to the survival of those which accidentally had slightly thicker skins; but partly, and I think chiefly, also to the powers of the skin as a complex tissue to resist the attacks of the many small enemies, such as animal and vegetable parasites, which are a constant element in almost all environments. In this case, also, with each generation or moult we have a fresh start; on the removal of the constant irritation from these enemies, the energies of the animal would be otherwise employed than in re-developing its shell.

In order to account for the progressive or retrogressive modifications in the sterile workers of Hymenopterous colonies, we have in the same way to assume the functional modification of the plastic young as the prime factor; Natural Selection not only perfects these modifications, but *drives them back earlier and earlier in the life histories of the individuals.* We can thus understand that a stage might be reached when the difference of the food administered to the larva might modify its course of development. The structural modifications of the workers in this case are not inherited, but they are also not primarily due to Natural Selection, but to the response of organisms *in ever-recurring plastic series* to the requirements of their environment.

Again, the ceaseless efforts of the young individuals of a colony to adapt themselves to a new environment can alone, it seems to me, account for the possible utilisation of congenital variations, which are useless in themselves without concomitant variations. These latter will be acquired by the efforts of the organism, so far, that is, as to render the former functionally useful.

On the other hand, the "time" difficulty in evolution admits of easy solution if it can be shown that groups of organisms actively respond to changes of environment, as plants adapt themselves to a new surface by the plasticity of their growing shoots. Further, the difficulty that organisms are known to have remained practically unchanged through immense geological periods, is explained by the supposition that their environment must have also remained practically unchanged.

For the solution of many of the difficulties in evolution, we have then to look to the functional response of *colonies* of organisms, and of the living parts of such organisms to their respective environments. This power of adjustment accounts not only for the formation, but also for the maintenance of species. The force of Heredity has been overstated, while the power of immediate vital response of delicately balanced organisms to every slight change in the environment has been very much understated. If we keep in view the ever-recurring generations of plastic young, the direct stimulus of the environment is seen almost necessarily to be a force of prime importance perpetually overmastering the somewhat exaggerated rigidity of species attributed to heredity. There is no such thing as rigidity; everything is rather in a state of flux. Is this unending variation, always in adjustment to the environment, due to Natural Selection taking advantage of the occasional accidental slips in an otherwise rigid heredity? or, is it due to the direct response of organisms in a state of finely balanced equilibrium? This latter seems to me the more probable, the resulting structural modifications being, on the one hand, hindered by Heredity; on the other, if the conditions require it, hastened and perfected by Natural Selection. This hastening action of Natural Selection leads inevitably to inheritance.

The evolutionary theories known as Lamarckism and Darwinism, only break down when they are supposed to be mutually exclusive. Is it not possible to unite them somewhat in the manner here suggested?

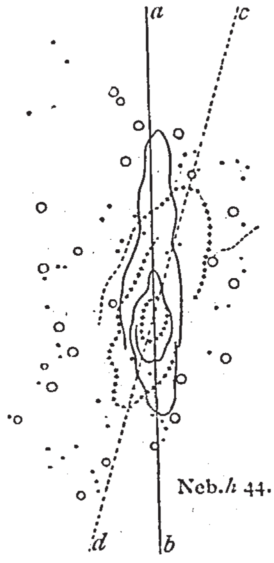
H. M. Bernard.

Natural History Museum, September 17.

The Great Nebula in Andromeda.

In reference to Dr. A. A. Common's review of Dr. Roberts' beautiful collection of celestial photographs (Nature, No. 1297, September 6), where he says "There is a fair presumption that in course of time the rotation of the outer portion (of the great Andromeda nebula) may perhaps be detected by observation of the positions of the two outer detached portions in relation to the neighbouring stars," I wish to point out that changes of this kind will perhaps be detected much sooner than it is generally expected.

In the accompanying drawing (presented May 2, 1894, to the *Société Astronomique de France*), *a–b* indicates the outline of the small elongated nebula *h* 44, as it was seen by Trouvelot at Cambridge (Mass.) in 1874; *c–d* the limits of the nebulosity



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on Roberts' photograph (1889); the circular spots are stars, recognisable in the drawing. Unless this part of the Trouvelot drawing—the excellence of which is stated by Dr. Roberts himself—be very incorrect, the nebula would seem to have turned about 15° from left to right. The globular nebula (M. 32) to the other side of M. 31, seems also to have slightly shifted its position.

Evidence of the reality of such changes is of course only obtainable by comparing three or more photographs taken at comparatively wide intervals. In the meantime, this short notice in Nature may call the attention of photo-astronomers to this interesting point.

Dordrecht, September 14. C. Easton.

On the Identification of Habitual Criminals by Finger-Prints.

A parliamentary Blue Book on "The Identification of Habitual Criminals," which has recently been issued, reports on *The Finger-Print System*, stated to have been "first suggested, and to some extent applied practically, by Sir William Herschel."

The chairman of the committee appointed by Mr. Asquith, whose report contains the above statement, refers me for his evidence on this point to Mr. Galton's work on "Finger-Prints" (Macmillan and Co., 1892).

My "careful study" of the subject is mentioned there, and an article of mine in Nature, October 28, 1880 (vol. xxii. p. 605), is referred to. It is correctly indexed in the "Index Medicus" for the year, published in 1881, although Mr. Galton spells and indexes my name incorrectly. That article, I believe, is absolutely the first notice of the subject contained in English literature, and the conclusion I reached therein was that the patterns of the skin-furrows, with their distinctive loops, whorls, and lines, breaking and blending like the junctions in a railway map, were capable of being readily used as a reliable and permanent basis for the "scientific identification of criminals." I conclude my paper with the statement that "There can be no doubt as to the advantage of having, besides their photographs, a nature-copy of the for-ever unchangeable finger-furrows of important criminals."

Sir William Herschel wrote in Nature, November 25 of the same year, alleging that he had "been taking sign-manuals by means of finger marks for now more than twenty years." It does not yet appear that anything had been published on the subject by that gentleman till my contribution called forth his letter a month afterwards. The collections made by Sir W. Herschel were recently placed in Mr. Galton's hands, and that writer states that "they refer to one or more fingers, and in a few instances to the whole hand, of fifteen different persons." ("Finger-Prints," p. 9.)

It is not stated how many of these had been imprinted prior to my first calling attention to the subject. At present it would seem that Sir. W. Herschel had not accumulated the impressions at a more rapid rate than that of one person in two years! As we are informed in the letter to Nature, referred to above, that the identification of pensioners had been secured in this way, that the method was in use in all the registration offices of the district, and that "on commitment to gaol, each prisoner had to sign with his finger," I should have expected that a somewhat more extensive collection might have been secured. As priority of publication is generally held to count for something, and as I knew absolutely nothing of Sir W. Herschel's studies, nor ever heard of anyone in India who did, some little evidence on the point of priority would be of interest even now.

Mr. Galton says, of Sir W. Herschel, "He informs me that he submitted, in 1877, a report in semi-official form to the Inspector-General of Gaols, asking to be allowed to extend the process; but no result followed." (p. 28.) A copy of that semi-official report would go far to settle the question of priority, as its date is nearly two years previous to my having noticed the finger-furrows. No reference to them was then to be found in any anatomical work that I could find access to, and no writer on identification had ever thought of them as a means to that end. My interest, like that of Purkenje, arose from a special study of the sense of touch, and I was then lecturing to medical students on the "Physiology of the Senses." Having myopic eyes which enable me to write with ease the Lord's Prayer three times in the space of a sixpence, I soon noticed the unique patterns which the papillary ridges formed. I happened to be studying the prehistoric pottery of Japan at the same time, and became interested in observing that these patterns were similar, but, I thought, finer and more slender than those of the present day, which pointed, I conjectured, to the employment of children in early fictile art. However that may be, my knowledge of the subject had a natural and independent genesis.

The subject of identification by this means has been brought under the notice of the authorities on criminal matters of different countries by me from time to time, and some years before Mr. Galton's work was published, Scotland Yard placed one of its most enlightened officers in communication with me on the subject. Inspector Tunbridge studied the subject with me during a forenoon. Even in 1880, I prepared copper-plate outlines of the two hands, accompanied with instructions as to obtaining finger-prints, and some two chief points on the palm, where the rugæ are characteristic. Sir W. Herschel's letter mentions prints of one finger only as being obtained from prisoners on commitment. On page 79 of the Blue Book mentioned above, "Instructions for taking Finger-Prints" are given for the benefit of prison warders, and the ten fingers are to be printed from, as I have advocated. I may add that I have not the slightest wish to diminish the credit that may be due to Sir W. Herschel. What I wish to point out is that his claim ought to be brought out a little more clearly than has yet been done, either by himself or by Mr. Galton. What precisely did he do, and when? Henry Faulds.

The Tetrahedral Carbon Atom.

Your reviewer, in his notice of my "Elementary Lessons in Organic Chemistry," takes exception to the statement that the carbon atom has been hypothetically regarded as tetrahedral in shape; he is presumably unacquainted with the criticisms of Lossen (*Berichte* 20, p. 3306) on Wislicenus's memoir, with Wislicenus's reply[1] (*Berichte* 21, p. 581), as well as with the pamphlet of Wunderlich ("Configuration organischer Moleküle," Würzburg, 1886); he need not, however, search "the whole range of stereo-chemical literature" for references of this kind, as there is in the "Handbuch" of V. Meyer and Jacobson, pp. 433-436, a tolerably full discussion as to the ultimate cause of stereo-isomerism in carbon compounds, where it is stated (p. 434) that "the carbon atom may be regarded as a mass of finite extension in space, of any shape, with four points on its surface corresponding to the corners of a regular tetrahedron as the units of affinity."

Most writers on stereo-chemical subjects prefer to speak of the tetrahedral arrangement in space of the four valencies of the carbon atom, rather than of the tetrahedral shape of the carbon atom itself; but if the "valencies" are sufficiently material to have a definite position in space, they may fairly be regarded as parts of the carbon atom, which then becomes of finite size, and *for the purposes of stereo-chemistry* essentially tetrahedral in shape. This form of statement has the merit of simplicity, and is in itself less objectionable than the idea of "valencies" directed towards the corners of a tetrahedron; at the same time, I freely admit that the statement errs on the side of excessive simplicity, and is not what would be adopted before a class of honours students.

It is possible to connect the facts of stereo-isomerism to some extent by a series of separate propositions, and at the same time to avoid any reference to the distribution of the "valencies" in space, or to the finite size of the carbon atom; one of these propositions would be that "two carbon atoms connected by an ethylene linkage are no longer free to rotate round the axis which joins them"; but so soon as an attempt is made to unite these separate statements into one hypothesis, or to assign any reason for the proposition just quoted, it is impossible (as it appears to me) to escape from the dilemma; of the two alternatives, I think most chemists, who have not become blinded by long usage to the gross misuse which the word "valency" has suffered, will prefer to regard the carbon atom as finite in size with four points in it, occupying the corners of a tetrahedron, distinguished in some way beyond the rest as regards the action of chemical affinity.

This idea must be made more definite before the average student can derive much help from it in correlating the facts of

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[1] Wislicenus says "the atom of carbon may possibly resemble very closely regular tetrahedron in shape."

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